or, what is the same thing, AA′ is expressible in the two forms $Md\omega$ and $M_1 d\omega_1$, BB′ in the two forms $M_1 d\omega_1$ and $M_2 d\omega_2$ &c., the identity of the two expressions for the same arc, of course, depending on the relation between the two parameters. But any such monomial expression $Md\omega$ of an arc AA′ would be of a complicated form, not obviously reducible to elliptic functions. Casey does not obtain them at all; but he finds geometrically monomial expressions for the differences and sum BB′−AA′, CC′−BB′, DD′+CC′, DD′−AA′ (they cannot be all of them differences), and thence a quadrinomial expression $AA'=N_1 d\omega_1+N_2 d\omega_2+N_3 d\omega_3+N d\omega$ (his $ds'=\rho d\theta+\rho' d\theta'+\rho'' d\theta''+\rho''' d\theta'''$), and that without any explicit consideration of the relations which connect the parameters.

I propose to complete the analytical theory by establishing the monomial equations $AA'=Md\omega=M_1 d\omega_1$, &c., and the relations between the parameters $\omega, \omega_1, \omega_2, \omega_3$, which belong to an inscribed quadrilateral ABCD, so as to show what the process really is by which we pass from the monomial form to a quadrinomial form $AA'=Nd\omega+N_1 d\omega_1+N_2 d\omega_2+N_3 d\omega_3$, $=d\mathrm{S}$, wherein each term is separately expressible as the differential of an elliptic integral, and to further develop the theory of the transformation to elliptic integrals.

V. "On the Influence of Height in the Atmosphere on the Diurnal Variation of the Earth's Magnetic Force." By J. A. Broun, F.R.S. Received January 25, 1877.

In a paper in the Society's Transactions on the earth's magnetic intensity at Bombay, Mr. C. Chambers has examined the question of the influence of height on the diurnal inequality of the horizontal force*. Two instruments were observed simultaneously at $0^h\ 22^m$ and $2^h\ 29^m$ P.M.: one, a bifilar magnetometer, was 6 feet above the ground; the other, a unifilar absolute-intensity instrument, was 38 feet above the ground.

Mr. Chambers has found that, in the interval between the two times specified, the change of horizontal force given by the bifilar magnetometer was rather more than one third (0·37) of the mean diurnal range, and that the absolute intensity instrument showed a change about one fifth less than the bifilar. This difference he does not think instrumental; and he considers that, if true, "it suggests the attribution of a very considerable magnetic influence to the state of the medium intervening between the upper and lower places of observation," &c.

The mean changes of horizontal force between $0^h\ 22^m$ and $2^h\ 29^m$ P.M. by the two instruments were as follow:—

* "The Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, &c." By Charles Chambers, F.R.S., Superintendent of the Colaba Observatory. Phil. Trans. 1876, p. 84.

By bifilar, 6 feet above ground ..... = − 0·00062 X,
By intensity unifilar, 38 „ „ „ ..... = − 0·00048 X,
Mean diurnal range by the bifilar ..... = 0·00166 X,

where X is the whole horizontal magnetic force.

Having occupied myself at different times during the last thirty years with questions relating to the magnetic and meteorological variations at different heights in the atmosphere, I have examined my observations of the horizontal magnetic force, as far as they have been reduced, with reference to Mr. Chambers's conclusion.

In 1847 I made two series of simultaneous observations on the highest point of the Cheviot range of hills, and at Makerstoun in Scotland, about 30 miles distant: the difference of height of the two stations is 2440 feet. The first series was made in June with a bifilar at Makerstoun, and an intensity unifilar on Cheviot*. The difference of the daily range of horizontal force at the two stations, as deduced from hourly observations during three days, was certainly less than one twentieth of the whole daily range; but part at least of this difference was probably due to instrumental causes.

For a second expedition in August of the same year (1847) the same instrument was employed on Cheviot; but a second intensity unifilar was observed at Makerstoun, in addition to the bifilar magnetometer. The observatory on the first expedition was under a tent, on the second it was under ground, a deep cutting having been made for this purpose. The result for the horizontal intensity at Makerstoun showed such differences between the two instruments used there (in the same room), that it was evident the unifilar intensity instrument could not be depended on for small differences in the amount of the variations.

In the years 1855 to 1858 two bifilar magnetometers of precisely the same construction in every way, devised by me expressly for these comparisons, were placed, one in the Agustia Malley Observatory, 6200 feet above the sea, the other in the Trevandrum Observatory (lat. 8° 31′ N.), 200 feet above the sea (about 24 miles from the former station).

The first year's observations of the bifilar in the peak observatory were found to be valueless *for this question*, as it was discovered that the thermometer, though in the same box, did not show with sufficient exactness the temperature of the magnet: this was due to the magnet being within a pasteboard box, to protect it more completely from currents of air, while the thermometer bulb was outside this inner box. This source of error was avoided by placing two thermometers with their bulbs on opposite sides of the magnet and within the inner box. The series of hourly observations after the middle of August 1856 is

* Some account of the results for the Magnetic Declination obtained from this expedition was given to the British Association immediately afterwards (see Brit. Assoc. Rep. 1847, p. 19).

believed to be free from all error. The unit-coefficients were determined with the greatest accuracy for both the instrument on the peak and that on the plain, verified by different methods, giving results which did not differ by one five-hundredth of the whole value. The temperature-coefficients were also found with much exactness; and, as hard steel magnets were chosen expressly for these instruments, the temperature-coefficients were small.

The following results from the hourly observations, made during the last four months of 1856, will be sufficient for my present object. Taking the observations at the hours nearest to those for which Mr. C. Chambers has obtained his result (namely, $0^h\ 30^m$ and $2^h\ 30^m$ P.M.), I find the mean change of horizontal force from the former to the latter time—

On the plain, 200 feet above the sea ..... $= -0{\cdot}0009760\,X$,
On the mountain, 6200 feet above the sea .... $= -0{\cdot}0009724\,X$;

so that the change on the mountain-peak was less than on the plain by one two-hundred-and-seventieth. In each of the months October and November the change was exactly the same at the two stations.

If we take double the interval, so as to keep the same hours in the middle, I find the mean change from $11^h\ 30^m$ A.M. to $3^h\ 30^m$ P.M.—

On the plain, 200 feet above the sea ..... $= -0{\cdot}0016556\,X$,
On the peak, 6200 feet above the sea ..... $= -0{\cdot}0016510\,X$,

The change was therefore one three-hundred-and-sixtieth less on the peak than on the plain. The mean of the diurnal ranges for the four months was $0{\cdot}00215\,X$.

It will be seen that in the interval of four hours the change was as great as the whole mean diurnal range at Bombay; and if the quantities had been given to five places of decimals only as for Bombay, the movements would have appeared exactly the same at the two stations.

It will thus be seen that instead of $\frac{1}{5}$ less for a difference of 32 feet in height, I do not find more than $\frac{1}{360}$ for a difference of 6000 feet when the change during four hours is considered; nor, till I have made a more searching investigation of the whole series of observations, can I vouch that this difference (which is very much less than the probable error of an observation at either station) is not accidental.

Another series of observations was made at the same two stations in 1864, when a unifilar horizontal-force magnetometer, on Dr. Lamont's construction, was employed, as well as the bifilar instrument. These observations are not yet completely discussed; and on that account I do not enter at present into the question as to what difference may exist in the laws of magnetic variations when the height differs by 6000 feet. Meanwhile it will be useful, I believe, to those who may attempt investi-

gations of this kind, to be acquainted with some of the causes of failure which I have met with, and to know how small the difference of the variations probably is when we ascend to a considerable height in the atmosphere. The results I have obtained from two instruments placed in positions so greatly different as those of the cloudy mountain-peak and the sunny plain, will also show the degree of accuracy attainable when the requisite precautions are taken, and accurate methods of correction and reduction have been employed.

VI. "On Heat as a Germicide when Discontinuously Applied." By John Tyndall, F.R.S. Received February 14, 1877.

Royal Institution, Feb. 14th, 1877.

My dear Huxley,—In my "Preliminary Note," communicated to the Royal Society on the 18th of January, various infusions were referred to as manifesting an astonishing resistance to sterilization by heat. This resistance was traced to its source; and I have been since informed that you were good enough to express at the time a very favourable opinion as to the significance and value of the results indicated.

It will, I think, now interest you to learn that the most obstinate of the infusions referred to in the "Note" have been since rendered tractable by the application of very simple means. Following up the plain suggestions of the germ theory, I have been able, even in the midst of a virulently infective atmosphere, to sterilize all the infusions by a temperature lower than that of boiling water.

It is known that the prolonged application of a low temperature is often equivalent to the brief application of a higher one; and you may therefore be disposed to conclude that in the experiments here referred to I have substituted time for intensity. This, however, is not the case. The result depends solely upon the manner in which the heat is applied. For example, I boil an infusion for fifteen minutes, expose it to a temperature of 90° Fahr., and find it twenty-four hours afterwards swarming with life. I submit a second sample of the same infusion to a temperature lower than that of boiling water for five minutes, and it is rendered permanently barren.

The secret of success here is an open one. I have already referred to the period of latency which precedes the clouding of infusions with visible *Bacteria*. During this period the germs are being prepared for their emergence into the finished organism. They reach the end of this period of preparation successively—the period of latency of any germ depending upon its condition as regards dryness and induration. This, then, is my mode of proceeding:—Before the latent period of any of the germs has been completed (say a few hours after the preparation of the infusion), I subject it for a brief interval to a temperature which may be